

Nonlin. Processes Geophys. Discuss., referee comment RC2  
<https://doi.org/10.5194/npg-2021-21-RC2>, 2021  
© Author(s) 2021. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## **Comment on npg-2021-21**

Anonymous Referee #2

---

Referee comment on "Enhanced diapycnal mixing with polarity-reversing internal solitary waves revealed by seismic reflection data" by Yi Gong et al., Nonlin. Processes Geophys. Discuss., <https://doi.org/10.5194/npg-2021-21-RC2>, 2021

---

## **Referee's Report on "Enhanced diapycnal mixing by polarity reversing internal solitary waves in the South China Sea" by Gong et al.**

### **General comments**

In this study, the seismic images are used to analyze the spatial distribution of internal waves in the Dongsha area at a shelf of the South China Sea. The seismic data on three survey lines were processed using modified by authors' methodology. The turbulence dissipation was estimated from obtained horizontal wavenumber slope spectrum of the reflection events. Then diapycnal diffusivity was calculated using the Osborn model.

Three conclusions of this work should be highlighted: 1) Enhanced level of diffusivity over shelf caused by breaking internal waves; 2) The diffusivity in the internal solitary wave with reversing polarity is three times that of the wave without reversing polarity; 3) Diffusivity and dissipation are greater in front of ISW.

The applied method is also of interest. The general comment is related to using chain assumptions, filtering windows, data from different sources (e.g. stratification parameters from Copernicus), etc. The question is: Would authors provide estimates of the sensitivity of calculated dissipation and diffusivity to the used in calculations parameters/assumptions because of the lack of comparison with dissipation/diffusivity data obtained using other methods?

The topics of the article are sufficiently novel and interesting to warrant publication in the Nonlinear Processes in Geophysics after minor revision.

## Minor comments

l. 26 " the difference between our and previous diffusivity profiles is about 2-3 orders of magnitude,...". May be better to write "mixing scheme based on Richardson number dependent turbulence parameterizations instead of "previous diffusivity profiles"?

l.64  $O(-3) \text{ m}^2 \text{ s}^{-1}$  Please, use standard order designation  $O(10^{-3})$  through the paper and correct misprint.

l.274 and l. 284 "Huntter" read as Hutter